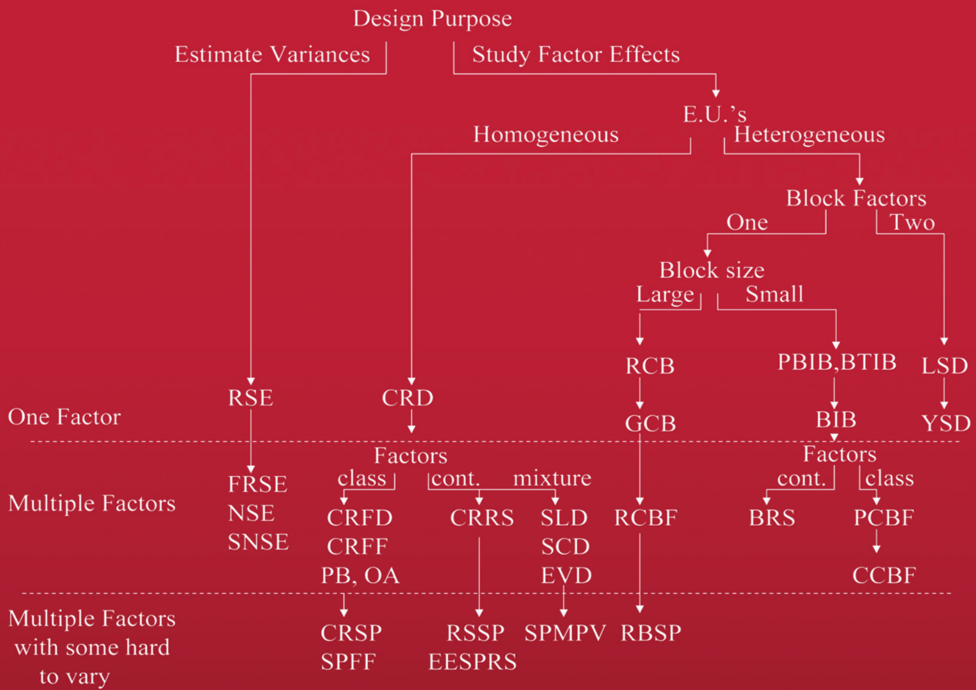
Texts in Statistical Science

Design and Analysis of Experiments with R



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CHAPTER 1

Introduction

1.1 Statistics and Data Collection

Statistics is defined as the science of collecting, analyzing, and drawing conclusions from data. Data is usually collected through sampling surveys, observational studies, or experiments.

Sampling surveys are normally used when the purpose of data collection is to estimate some property of a finite population without conducting a complete census of every item in the population. For example, if there were interest in finding the proportion of registered voters in a particular precinct that favor a proposal, this proportion could be estimated by polling a random sample of voters rather than questioning every registered voter in the precinct.

Observational studies and experiments, on the other hand, are normally used to determine the relationship between two or more measured quantities in a conceptual population. A conceptual population, unlike a finite population, may only exist in our minds. For example, if there were interest in the relationship between future greenhouse gas emissions and future average global temperature, the population, unlike registered voters in a precinct, cannot be sampled from because it does not yet exist.

To paraphrase the late W. Edwards Deming, the value of statistical methods is to make predictions which can form the basis for action. In order to make accurate future predictions of what will happen when the environment is controlled, cause and effect relationships must be assumed. For example, to predict future average global temperature given that greenhouse gas emissions will be controlled at a certain level, we must assume that the relationship between greenhouse gas emissions and global temperature is cause and effect. Herein lies the main difference in observational studies and experiments. In an observational study, data is observed in its natural environment, but in an experiment the environment is controlled. In observational studies it cannot be proven that the relationships detected are cause and effect. Correlations may be found between two observed variables because they are both affected by changes in a third variable that was not observed or recorded, and any future predictions made based on the relationships found in an observational study must assume the same interrelationships among variables that existed in the past will exist in the future. In an experiment, on the other hand, some variables are purposely changed while others are held constant. In that way the effect that is caused by the change in the purposely varied variable can be directly observed, and predictions can be made about the result of future changes to the purposely varied variable.

1.2 Beginnings of Statistically Planned Experiments

There are many purposes for experimentation. Some examples include: determining the cause for variation in measured responses observed in the past; finding conditions that give rise to the maximum or minimum response; comparing the response between different settings of controllable variables; and obtaining a mathematical model to predict future response values.

Presently, planned experiments are used in many different fields of application such as: engineering design, quality improvement, industrial research and manufacturing, basic research in physical and biological science, research in social sciences, psychology, business management and marketing research, and many more. However, the roots of modern experimental design methods stem from R. A. Fisher's work in agricultural experimentation at the Rothamsted Experimental Station near Harpenden, England.

Fisher was a gifted mathematician whose first paper as an undergraduate at Cambridge University introduced the theory of likelihood. He was later offered a position at University College, but turned it down to join the staff at Rothamsted in 1919. There, inspired by daily contact with agricultural research, he not only contributed to experimental studies in areas such as crop yields, field trials, and genetics, but also developed theoretical statistics at an astonishing rate. He also came up with the ideas for planning and analysis of experiments that have been used as the basis for valid inference and prediction in various fields of application to this day. Fisher (1926) first published his ideas on planning experiments in his paper "The arrangement of field experiments"; 9 years later he published the first edition of his book *The Design of Experiments*, Fisher (1935).

The challenges that Fisher faced were the large amount of variation in agricultural and biological experiments that often confused the results, and the fact that experiments were time consuming and costly to carry out. This motivated him to find experimental techniques that could:

- eliminate as much of the natural variation as possible
- prevent unremoved variation from confusing or biasing the effects being tested
- detect cause and effect with the minimal amount of experimental effort necessary.

1.3 Definitions and Preliminaries

Before initiating an extended discussion of experimental designs and the planning of experiments, I will begin by defining the terms that will be used frequently.

- *Experiment* (also called a *Run*) is an action where the experimenter changes at least one of the variables being studied and then observes the effect of his or her actions(s). Note the passive collection of observational data is not experimentation.
- *Experimental Unit* is the item under study upon which something is changed. This could be raw materials, human subjects, or just a point in time.
- *Sub-Sample, Sub-Unit, or Observational Unit* When the experimental unit is split, after the action has been taken upon it, this is called a sub-sample or sub-unit. Sometimes it is only possible to measure a characteristic separately for each sub-unit; for that reason they are often called observational units. Measurements on sub-samples, or sub-units of the same experimental unit, are usually correlated and should be averaged before analysis of data rather than being treated as independent outcomes. When sub-units can be considered independent and there is interest in determining the variance in sub-sample measurements, while not confusing the F-tests on the treatment factors, the mixed model described in Section 5.8 should be used instead of simply averaging the sub-samples.
- *Independent Variable* (*Factor* or *Treatment Factor*) is one of the variables under study that is being controlled at or near some target value, or *level*, during any given experiment. The level is being changed in some systematic way from run to run in order to determine what effect it has on the response(s).
- *Background Variable* (also called a *Lurking Variable*) is a variable that the experimenter is unaware of or cannot control, and which could have an effect on the outcome of the experiment. In a well-planned experimental design, the effect of these lurking variables should balance out so as to not alter the conclusion of a study.
- *Dependent Variable* (or the *Response* denoted by Y) is the characteristic of the experimental unit that is measured after each experiment or run. The magnitude of the response depends upon the settings of the independent variables or factors and lurking variables.
- *Effect* is the change in the response that is caused by a change in a factor or independent variable. After the runs in an experimental design are conducted, the effect can be estimated by calculating it from the observed response data. This estimate is called the *calculated effect.* Before the experiments are ever conducted, the researcher may know how large the effect should be to have practical importance. This is called a *practical effect* or the *size of a practical effect.*
- *Replicate* runs are two or more experiments conducted with the same settings of the factors or independent variables, but using different experimental units. The measured dependent variable may differ among replicate runs due to changes in lurking variables and inherent differences in experimental units.

- *Duplicates* refer to duplicate measurements of the same experimental unit from one run or experiment. The measured dependent variable may vary among duplicates due to measurement error, but in the analysis of data these duplicate measurements should be averaged and not treated as separate responses.
- *Experimental Design* is a collection of experiments or runs that is planned in advance of the actual execution. The particular runs selected in an experimental design will depend upon the purpose of the design.
- *Confounded Factors* arise when each change an experimenter makes for one factor, between runs, is coupled with an identical change to another factor. In this situation it is impossible to determine which factor causes any observed changes in the response or dependent variable.
- *Biased Factor* results when an experimenter makes changes to an independent variable at the precise time when changes in background or lurking variables occur. When a factor is biased it is impossible to determine if the resulting changes to the response were caused by changes in the factor or by changes in other background or lurking variables.
- *Experimental Error* is the difference between the observed response for a particular experiment and the long run average of all experiments conducted at the same settings of the independent variables or factors. The fact that it is called "error" should not lead one to assume that it is a mistake or blunder. Experimental errors are not all equal to zero because background or lurking variables cause them to change from run to run. Experimental errors can be broadly classified into two types: bias error and random error. Bias error tends to remain constant or change in a consistent pattern over the runs in an experimental design, while random error changes from one experiment to another in an unpredictable manner and average to be zero. The variance of random experimental errors can be obtained by including replicate runs in an experimental design.

With these definitions in mind, the difference between observational studies and experiments can be explained more clearly. In an observational study, variables (both independent and dependent) are observed without any attempt to change or control the value of the independent factors. Therefore any observed changes in the response, or dependent variable, cannot necessarily be attributed to observed changes in the independent variables because background or lurking variables might be the cause. In an experiment, however, the independent variables are purposely varied and the runs are conducted in a way to balance out the effect of any background variables that change. In this way the average change in the response can be attributed to the changes made in the independent variables.

1.4 Purposes of Experimental Design

The use of experimental designs is a prescription for successful application of the scientific method. The scientific method consists of iterative application of the following steps: (1) observing of the state of nature, (2) conjecturing or hypothesizing the mechanism for what has been observed, then (3) collecting data, and (4) analyzing the data to confirm or reject the conjecture. Statistical experimental designs provide a plan for collecting data in a way that they can be analyzed statistically to corroborate the conjecture in question. When an experimental design is used, the conjecture must be stated clearly and a list of experiments proposed in advance to provide the data to test the hypothesis. This is an organized approach which helps to avoid false starts and incomplete answers to research questions.

Another advantage to using the experimental design approach is the ability to avoid confounding factor effects. When the research hypothesis is not clearly stated and a plan is not constructed to investigate it, researchers tend toward a trial and error approach wherein many variables are simultaneously changed in an attempt to achieve some goal. When this is the approach, the goal may sometimes be achieved, but it cannot be repeated because it is not known what changes actually caused the improvement.

One of Fisher's early contributions to the planning of experiments was popularizing a technique called randomization, which helps to avoid confusion or biases due to changes in background or lurking variables. As an example of what we mean by bias is "The Biggest Health Experiment Ever," Meier (1972), wherein a trial of a polio vaccine was tested on over 1.8 million children. An initial plan was proposed to offer vaccinations to all children in the second grade in participating schools, and to follow the polio experience of first through third graders. The first and third grade group would serve as a "control" group. This plan was rejected, however, because doctors would have been aware that the vaccine was only offered to second graders. There are vagaries in the diagnosis of the majority of polio cases, and the polio symptoms of fever and weakness are common to many other illnesses. A doctor's diagnosis could be unduly influenced by his knowledge of whether or not a patient had been vaccinated. In this plan the factor purposely varied, vaccinated or not, was biased by the lurking variable of doctors' knowledge of the treatment.

When conducting physical experiments, the response will normally vary over replicate runs due solely to the fact that the experimental units are different. This is what we defined to be experimental error in the last section. One of the main purposes for experimental designs is to minimize the effect of experimental error. Aspects of designs that do this, such as randomization, replication, and blocking, are called methods of *error control*. Statistical methods are used to judge the average effect of varying experimental factors against the possibility that they may be due totally to experimental error. Another purpose for experimental designs is to accentuate the factor effects (or signal). Aspects of designs that do this, such as choice of the number and spacing of factor levels and factorial plans, are called methods of *treatment design*. How this is done will be explained in the following chapters.

1.5 Types of Experimental Designs

There are many types of experimental designs. The appropriate one to use depends upon the objectives of the experimentation. We can classify objectives into two main categories. The first category is to study the sources of variability, and the second is to establish cause and effect relationships. When variability is observed in a measured variable, one objective of experimentation might be to determine the cause of that variation. But before cause and effect relationships can be studied, a list of independent variables must be determined. By understanding the source of variability, researchers are often led to hypothesize what independent variables or factors to study. Thus experiments to study the source of variability are often a starting point for many research programs. The type of experimental design used to classify sources of variation will depend on the number of sources under study. These alternatives will be presented in Chapter 5.

The appropriate experimental design that should be used to study cause and effect relationships will depend on a number of things. Throughout the book the various designs are described in relation to the purpose for experimentation, the type and number of treatment factors, the degree of homogeneity of experimental units, the ease of randomization, and the ability to block experimental units into more homogeneous groups. After all the designs are presented, Chapter 13 describes how they can be used in sequential experimentation strategies where knowledge is increased through different stages of experimentation. Initial stages involve discovering what the important treatment factors are. Later, the effects of changing treatment factors are quantified, and in final stages, optimal operating conditions can be determined. Different types of experimental designs are appropriate for each of these phases.

Screening experiments are used when the researcher has little knowledge of the cause and effect relationships, and many potential independent variables are under study. This type of experimentation is usually conducted early in a research program to identify the important factors. This is a critical step, and if it is skipped, the later stages of many research programs run amuck because the important variables are not being controlled or recorded.

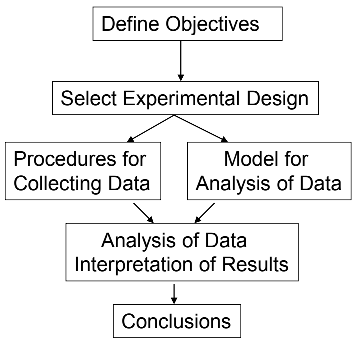
After identifying the most important factors in a screening stage, the researcher's next objective would be to choose between constrained optimization or unconstrained optimization (see Lawson, 2003). In constrained optimization there are usually six or fewer factors under study and the purpose is to quantify the effects of the factors, interaction or joint effects of factors, and to identify optimum conditions among the factor combinations actually tested.

When only a few quantitative factors are under study and curvilinear relationships with the response are possible, it may be possible to identify improved operating conditions by interpolating within the factor levels actually tested. If this is the goal, the objective of experimentation is called unconstrained optimization. With an unconstrained optimization objective, the researcher is normally trying to map the relationship between one or more responses and five or fewer quantitative factors.

Specific experimental design plans for each of the stages of experimentation will be presented as we progress through the book.

Figure 1.1 shows the relationship between the objectives of experimentation, the design of the experiment, and the conclusions that can be drawn. The objective of a research program dictates which type of experimental design should be utilized. The experimental design plan in turn specifies how the data should be collected and what mathematical model should be fit in order to analyze and interpret the data. Finally, the type of data and the mathematical model will determine what possible conclusions can be drawn from the experiment. These steps are inseparable and dependent upon each other. Many mistakes are made in research by trying to dissever these steps. An appropriate analysis of data cannot be completed without knowledge of what experimental design was used and how the data was collected, and conclusions are not reliable if they are not justified by the proper modeling and analysis of the data.

Figure 1.1 *Objectives, Design, and Conclusions from Experimentation*



1.6 Planning Experiments

An effective experimental design plan should include the following items: (1) a clear description of the objectives, (2) an appropriate design plan that guarantees unconfounded factor effects and factor effects that are free of bias, (3) a provision for collecting data that will allow estimation of the variance of the experimental error, and (4) a stipulation to collect enough data to satisfy the objectives. Bisgaard (1999) recommends a formal proposal to ensure that a plan includes all of these elements. The proposal should include a checklist for planning the experiments. Below is a checklist that is similar to Bisgaard's. Examples of some of the steps from this checklist will be illustrated in discussing a simple experiment in the next section.

1. *Define Objectives.* Define the objectives of the study. First, this statement should answer the question of why is the experiment to be performed. Second, determine if the experiment is conducted to classify sources of variability or if its purpose is to study cause and effect relationships. If it is the latter, determine if it is a screening or optimization experiment. For studies of cause and effect relationships, decide how large an effect should be in order to be meaningful to detect.
2. *Identify Experimental Units.* Declare the item upon which something will be changed. Is it an animal or human subject, raw material for some processing operation, or simply the conditions that exist at a point in time or *trial*? Identifying the experimental units will help in understanding the experimental error and variance of experimental error.
3. *Define a Meaningful and Measurable Response or Dependent Variable*. Define what characteristic of the experimental units can be measured and recorded after each run. This characteristic should best represent the expected differences to be caused by changes in the factors.
4. *List the Independent and Lurking Variables.* Declare which independent variables you wish to study. Ishikawa Cause and Effect Diagrams (see SAS Institute, 2004b) are often useful at this step to help organize variables thought to affect the experimental outcome. Be sure that the independent variables chosen to study can be controlled during a single run, and varied from run to run. If there is interest in a variable, but it cannot be controlled or varied, it cannot be included as a factor. Variables that are hypothesized to affect the response, but cannot be controlled, are lurking variables. The proper experimental design plan should prevent uncontrollable changes in these variables from biasing factor effects under study.
5. *Run Pilot Tests.* Make some pilot tests to be sure you can control and vary the factors that have been selected, that the response can be measured, and that the replicate measurements of the same or similar experimental units are consistent. Inability to measure the response accurately or to control the factor levels are the main reasons that experiments fail to produce desired results. If the pilot tests fail, go back to steps 2, 3, and 4. If these tests are successful, measurements of the response for a few replicate tests with the same levels of the factors under study will produce data that can be used to get a preliminary estimate of the variance of experimental error.
6. *Make a Flow Diagram of the Experimental Procedure for Each Run.* This will make sure the procedure to be followed is understood and will be standardized for all runs in the design.

7. *Choose the Experimental Design.* Choose an experimental design that is suited for the objectives of your particular experiment. This will include a description of what factor levels will be studied and will determine how the experimental units are to be assigned to the factor levels or combination of factor levels if there are more than one factor. One of the plans described in this book will almost always be appropriate. The choice of the experimental design will also determine what model should be used for analysis of the data.
8. *Determine the Number of Replicates Required.* Based on the expected variance of the experimental error and the size of a practical difference, the researcher should determine the number of replicate runs that will give a high probability of detecting an effect of practical importance.
9. *Randomize the Experimental Conditions to Experimental Units.* According to the particular experimental design being used, there is a proscribed method of randomly assigning experimental conditions to experimental units. In some designs, factor levels or combination of factor levels are assigned to experimental units completely at random. In other designs, randomizing factor levels is performed separately within groups of experimental units and may be done differently for different factors. The way the randomization is done affects the way the data should be analyzed, and it is important to describe and record exactly what has been done. The best way to do this is to provide a data collection worksheet arranged in the random order in which the experiments are to be collected. For more complicated experimental designs Bisgaard (1999) recommends one sheet of paper describing the conditions of each run with blanks for entering the response data and recording observations about the run. All these sheets should then be stapled together in booklet form in the order they are to be performed.
10. *Describe a Method for Data Analysis.* This should be an outline of the steps of the analysis. An actual analysis of simulated data is often useful to verify that the proposed outline will work.
11. *Timetable and Budget for Resources Needed to Complete the Experiments.* Experimentation takes time and having a schedule to adhere to will improve the chances of completing the research on time. Bisgaard (1999) recommends a Gantt Chart (see SAS Institute, 2004a), which is a simple graphical display showing the steps of the process as well as calendar times. A budget should be outlined for expenses and resources that will be required.

1.7 Performing the Experiments

In experimentation, careful planning and execution of the plan are the most important steps. As we know from Murphy's Law, if anything can go wrong it will, and analysis of data can never compensate for botched experiments. To illustrate the potential problems that can occur, consider a simple experiment conducted by an amateur gardener described by Box *et al.* (1978). The purpose was to determine whether a change in the fertilizer mixture would result in a change in the yield of his tomato plants. Eleven tomato plants were planted in a single row, and the fertilizer type (A or B) was varied. The experimental unit in this experiment is the tomato plant plus the soil it is planted in, and the treatment factor is the type of fertilizer applied. Easterling (2004) discusses some of the nuances that should be considered when planning and carrying out such a simple experiment. It is instructive to think about these in context with the checklist presented in the last section.

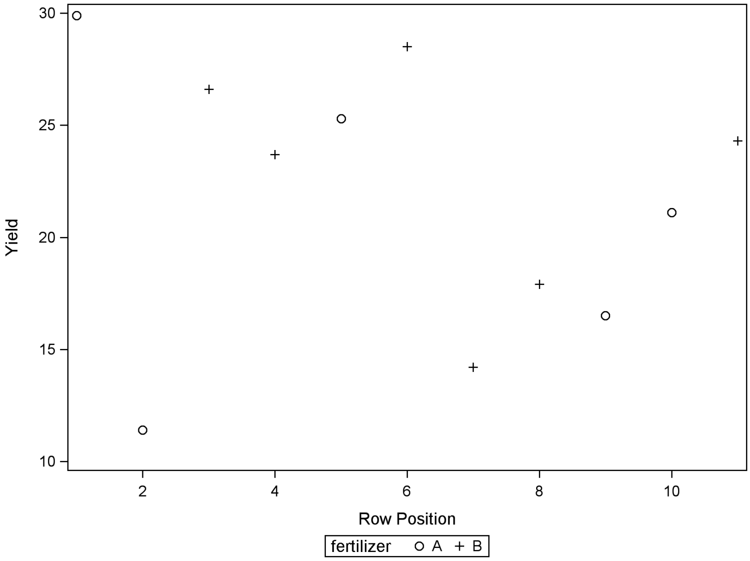
When defining the objectives for this experiment, the experimenter needs to think ahead to the possible implications of conclusions that he can draw. In this case, the possible conclusions are (1) deciding that the fertilizer has no effect on the yield of tomatoes, or (2) concluding that one fertilizer produces a greater yield. If the home gardener finds no difference in yield, he can choose to use the less expensive fertilizer. If he finds a difference, he will have to decide if the increase in yield offsets any increase in cost of the better fertilizer. This can help him determine how large a difference in yield he should look for and the number of tomato plants he should include in his study. The answer to this question, which is crucial in planning the experiment, would probably be much different for a commercial grower than for a backyard enthusiast.

The experimental units for this experiment were defined in the paragraph above, but in identifying them, the experimenter should consider the similarity or homogeneity of plants and how far apart he is going to place the tomato plants in the ground. Will it be far enough that the fertilizer applied to one plant does not bleed over and affect its neighbors?

Defining a meaningful response that can be measured may be tricky in this experiment. Not all the tomatoes on a single plant ripen at the same time. Thus, to measure the yield in terms of weight of tomatoes, the checklist and flow diagram describing how an experiment is conducted must be very precise. Is it the weight of all tomatoes on the plant at a certain date, or the cumulative weight of tomatoes picked over time as they ripen? Precision in the definition of the response and consistency in adherence to the definition when making the measurements are crucial.

There are many possible lurking variables to consider in this experiment. Any differences in watering, weeding, insect treatment, the method and timing of fertilizer application, and the amount of fertilizer applied may certainly affect the yield; hence the experimenter must pay careful attention to these variables to prevent bias. Easterling (2004) also pointed out that the row position seems to have affected the yield as well (as can be seen in Figure 1.2). The randomization of fertilizers to plants and row positions should equalize these differences for the two fertilizers. This was one of the things that Box *et al.* (1978) illustrated with this example. If a convenient method of applying the fertilizers (such as A at the beginning of the row followed by B) had been used in place of random assignment, the row position effect could have been mistaken for a treatment effect. Had this row position effect been known before the experiment was planned, the adjacent pairs of plots could have been grouped together in pairs, and one fertilizer assigned at random to one plot-plant in each pair to prevent bias from the row position effect. This technique is called blocking and will be discussed in detail in Chapter 4.

Figure 1.2 *Plot of Yield by Row Position—Tomato Experiment*



Easterling (2004) also raised the question: why were only eleven plants used in the study (five fertilized with fertilizer A and six with fertilizer B)? Normally flats of tomato plants purchased from a nursery come in flats of twelve. Was one plant removed from the study because it appeared unhealthy or got damaged in handling? The yield for the plant in the second row position (see Figure 1.2) of the 11 plants used was considerably lower than the others planted in neighboring row positions with the same fertilizer. Was this plant unhealthy or damaged as well?

Any problems that arise during the conduct of experiments should be carefully observed, noted, and recorded as comments on the data collection form described in step 9 of the checklist. Perhaps if this had been done for the tomato experiment, the low yield at row position two could be explained.

This discussion of a very simple experiment helps to emphasize the importance of carefully considering each step of the checklist presented in Section 1.6, and the importance of strict adherence to a flowchart for conducting the experiments, described in step 6 of that checklist. Failing to consider each point of the checklist, and inconsistency in conducting experiments and recording results, may lead to the demise of an otherwise useful research project.

1.8 Use of R Software

Fisher's original book on experimental designs clearly laid the logical principles for experimentation, but users of experimental designs needed to have more detailed descriptions of the most useful designs along with accompanying plans. Consulting statisticians needed to have a systematic explanation of the relation between experimental designs and the statistical theory of least squares and linear hypotheses, and to have an enumeration of designs and descriptions of experimental conditions where each design was most appropriate.

These needs were satisfied by Cochran and Cox (1950) and Kempthorne (1952) books. However, Cochran and Cox and Kempthorne's books were published before the age of computers and they both emphasize extensive tables of designs, abundant formulas, and numerical examples describing methods of manual analysis of experimental data and mathematical techniques for constructing certain types of designs. Since the publication of these books, use of experimental designs has gone far beyond agricultural research where it was initially employed, and a plethora of new books have been written on the subject. Even though computers and software (to both design and analyze data from experiments) are widely available, a high proportion of the more recent books on experimental design still follow the traditional pattern established by Cochran and Cox and Kempthorne by presenting extensive tables of designs and formulas for hand calculations and methods for constructing designs.

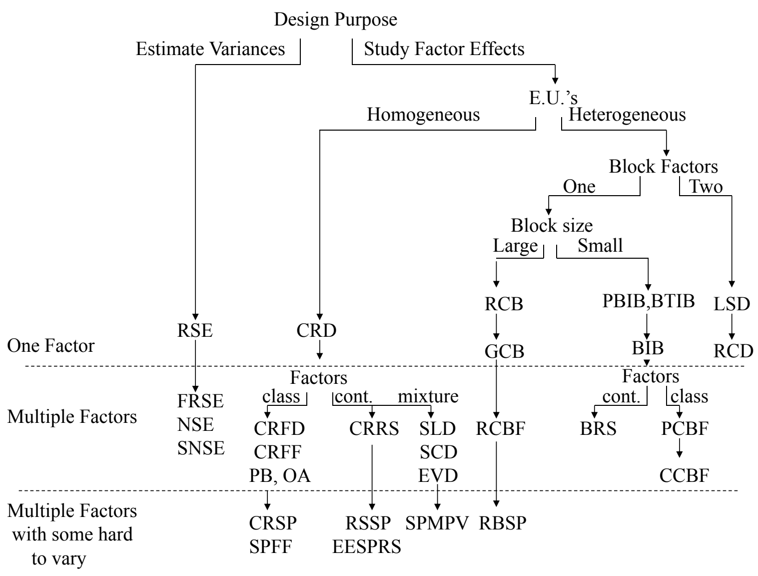
One of the objectives of this book is to break from the tradition and present computer code and output in place of voluminous formulas and tables. This will leave more room in the text to discuss the appropriateness of various design plans and ways to interpret and present results from experiments. The particular computer software illustrated in this book is R (R Development Core Team, 2003; Ihaka and Gentleman, 1996). In addition to R programing statements that are useful for constructing experimental designs and base functions that are useful for the analysis of experimental data, there are many user written packages that ease the construction of specific designs and provide analysis routines that are not available in the base R. These user written packages can be installed from CRAN. Packages illustrated in this book include: `agricolae`, `AlgDesign`, `BsMD`, `car`, `daewr`, `DoE.base`, `FrF2`, `GAD`, `gmodels`, `leaps`, `lme4`, `lsmeans`, `mixexp`, `multcomp`, and `Vdgraph`. An appendix is included at the end of the book with a brief introduction to R and additional references on using R.

1.9 Review of Important Concepts

This chapter describes the purpose for experimental designs. In order to determine if cause and effect relationships exist, an experimental design must be conducted. In an experimental design, the factors under study are purposely varied and the result is observed. This is different from observational studies or sampling surveys where data is collected with no attempt to control the environment. In order to predict what will happen in the future, when the environment is controlled, you must rely on cause and effect relationships. Relationships obtained from observational studies or sampling surveys are not reliable for predicting future results when the environment is to be controlled.

Experimental designs were first developed in agricultural research, but are now used in all situations where the scientific method is applied. The basic definitions and terminology used in experimental design are given in this chapter along with a checklist for planning experiments. In practice there are many different types of experimental designs that can be used. Which design is used in a particular situation depends upon the research objectives and the experimental units. Figure 1.3 is a diagram that illustrates when the different experimental designs described in this book should be used. As different experimental designs are presented in chapters to follow, reference will be made back to this figure to describe when the designs should be used.

Figure 1.3 *Design Selection Roadmap*



1.9.1 Design Name Acronym Index

RSE — random sampling experiment
FRSE — factorial random sampling experiment
NSE — nested sampling experiment

SNSE — staggered nested sampling experiment
CRD — completely randomized design
CRFD — completely randomized factorial design
CRFF — completely randomized fractional factorial
PB — Plackett-Burman design
OA — orthogonal array design
CRSP — completely randomized split plot
RSSP — response surface split plot
EESPRS — equivalent estimation split-plot response surface
SLD — simplex lattice design
SCD — simplex centroid design
EVD — extreme vertices design
SPMPV — split-plot mixture process variable design
RCB — randomized complete block
GCB — generalized complete block
RCBF — randomized complete block factorial
RBSP — randomized block split plot
PBIB — partially balanced incomplete block
BTIB — balanced treatment incomplete block
BIB — balance incomplete block
BRS — blocked response surface
PCBF — partially confounded blocked factorial
CCBF — completely confounded blocked factorial
LSD — Latin-square design
RCD — row-column design